A DESCRIPTION OF TWO METHODS,

BY WHICH

The IRREGULARITIES in the Motion of a CLOCK, ariſing from the Influence of Heat and Cold upon the Rod of the Pendulum, may be prevented.

By JOHN ELLICOTT, *F.R.S.*

Read at the ROYAL SOCIETY, *June* 4, 1752.

To which are added

A Collection of Papers,

Relating to the ſame SUBJECT,

Moſt of which were read at ſeveral Meetings of the *Royal Society*.

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TO THE

READER.

IT may not be improper, by way of Introduction, to give ſome general account of theſe papers, and the reaſon of their being made publick.

In June, 1752, I preſented to the Royal Society a Deſcription of Two Methods, by which the irregularities in the motion of a clock, ariſing from the influence of heat and cold upon the rod of the pendulum, might be prevented. One of theſe methods conſiſts in the particular conſtruction of the pendulum itſelf, and which I had made uſe of for ſeveral years, without ever having found it liable to thoſe objections which had been (and by ſome particular perſons ſtill are) made againſt it. But being unwilling to reſt it upon my own Opinion, though ſupported by a variety of experiments made on purpoſe, I delay'd communicating this invention to the Society, till it had undergone a trial of above two years, and had met with the approbation of a gentleman of known abilities and veracity, and who could in no wiſe be ſuppoſed to be prejudiced in favour of it, but as he expreſsly declares was rather prejudiced againſt it. And as in this paper I have confined myſelf entirely to facts relating to my own invention, without concerning myſelf in the leaſt with what had been done by others upon the ſame ſubject, I make no doubt, but that, upon a fair and candid perufal, it will not be found to deſerve any of thoſe cavils which have been raiſed againſt it. This paper was printed in the 47th vol. of Philoſophical Tranſactions, art. 71, page 479.

In November, 1752, Mr. James Short, F.R.S. preſented to the Royal Society a letter concerning the contrivance of the pendulum of a clock to prevent the irregularities of its motion by heat and cold. This Hiſtorical Account (as it is ſtiled) being upon a ſubject foreign to Mr. Short's profeſſion, and with which he may be ſuppoſed (without reflection upon his character as a Teleſcope maker) not to be thoroughly acquainted, it might therefore have been expected he ſhould have taken particular care, and have omitted no opportunities of furniſhing himſelf with proper materials for an hiſtory worthy the notice of the Royal Society; but the little pains he ſeems to have taken in this reſpect, the particular facts he has made choice of, and his manner of relating them, the time of communicating them, with ſeveral other circumſtances, which could hardly eſcape being taken notice of upon the bare reading of it, inclined ſeveral of my friends to be of opinion, that this performance was calculated to caſt a tacit blemiſh, and to prejudice me in my reputation and buſineſs. A paper was therefore drawn up by Mr. Edward Sparke, intitled, Some Remarks upon Mr. Short's Hiſtorical Account of the Contrivances in the Pendulum of a Clock, *&c.* This paper was read at a meeting of the Society, December the 7th, 1752, which was before Mr. Short's Hiſtorical Account was ordered to be printed; and it was expected that in common juſtice theſe *Remarks* ſhould be printed, to accompany the ſaid *Hiſtorical Account*, in caſe that ſhould be publiſhed in the Tranſactions of the Royal Society. But upon the publication of the 47th volume it appears, that the *Hiſtorical Account* alone is printed, and not the leaſt notice taken of the *Remarks*, and therefore in compliance with the opinion of my friends, and to do myſelf juſtice, I have now publiſhed thoſe *Remarks*, beginning page 23, and alſo the reſt of the papers herein contained.

The fourth paper herein publiſhed is entitled *Obſervations* on ſome Remarks on Mr. Short's Paper, read November 9, 1752, contained in a paper of Mr. Sparke, read December 7, 1752, by James Short. Read at the Royal Society, December 14, 1752. But this could not, with any propriety, be publiſhed in the 47th volume before-mentioned, as Mr. Sparke's *Remarks* were omitted. With theſe *Obſervations* are printed ſome *Notes* made by Mr. Sparke, ſince the publication of the Tranſactions.

The fifth of theſe papers, beginning at page 47, is a *Reply* to Mr. Short's Obſervations upon Mr. Sparke's Remarks, by J. Ellicott, which was read December 21, 1752, but could not be printed in the Tranſactions for the reaſons above-mentioned.

The ſixth and ſeventh papers, beginning at page 51, being a late letter I wrote to Mr. Short, and his anſwer, were occaſioned by a paſſage in Mr. Short's Obſervations, page 43, and ſerve to introduce the two following letters.

The eighth is a letter from the reverend Mr. Profeſſor Bliſs, to Mr. Short, quoted in part only in Mr. Short's Anſwer above-mentioned.

The ninth, beginning at page 55, is another letter from the ſame gentleman, being a farther anſwer to the objections raiſed by Mr. Short.

The Appendix contains the deſcription of the Pyrometer, mentioned in the firſt of theſe papers, which deſcription was formerly read to the Royal Society in April 1736, and afterwards printed in the Philoſophical Tranſactions, number 443, page 297.

I ſhall decline making any obſervations upon theſe papers, but ſubmit them entirely to the judgment of the candid and impartial reader.

J. ELLICOTT.

Fig. 1.

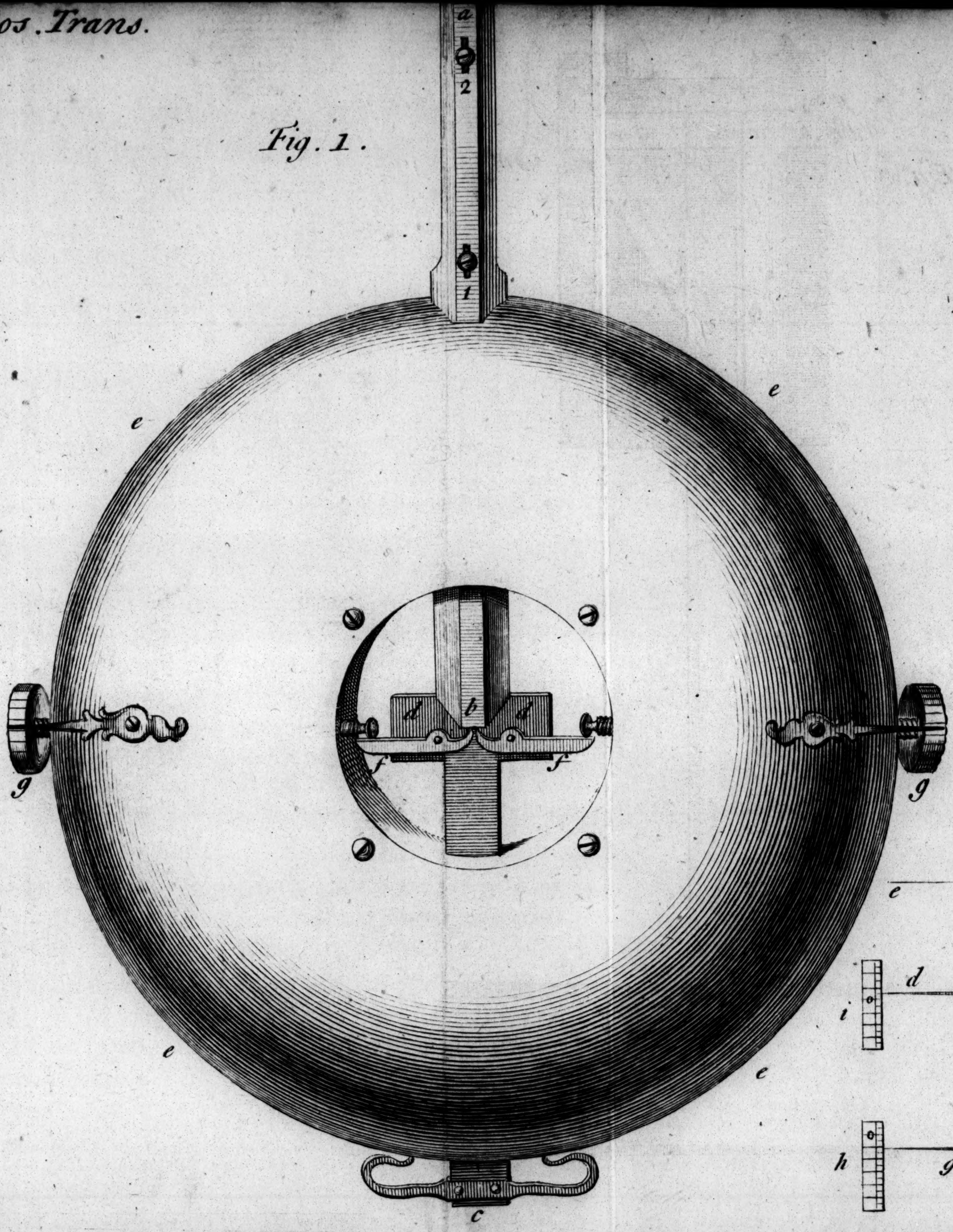
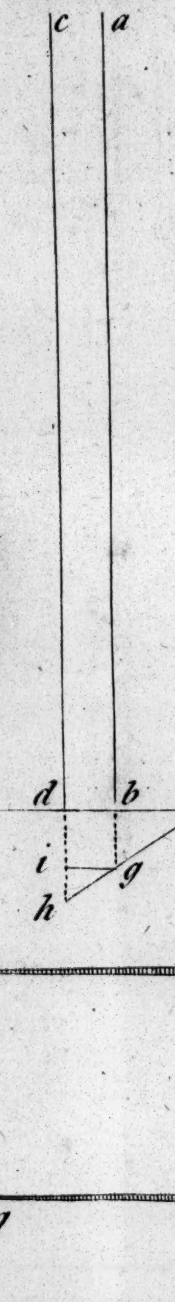


Fig. 2.



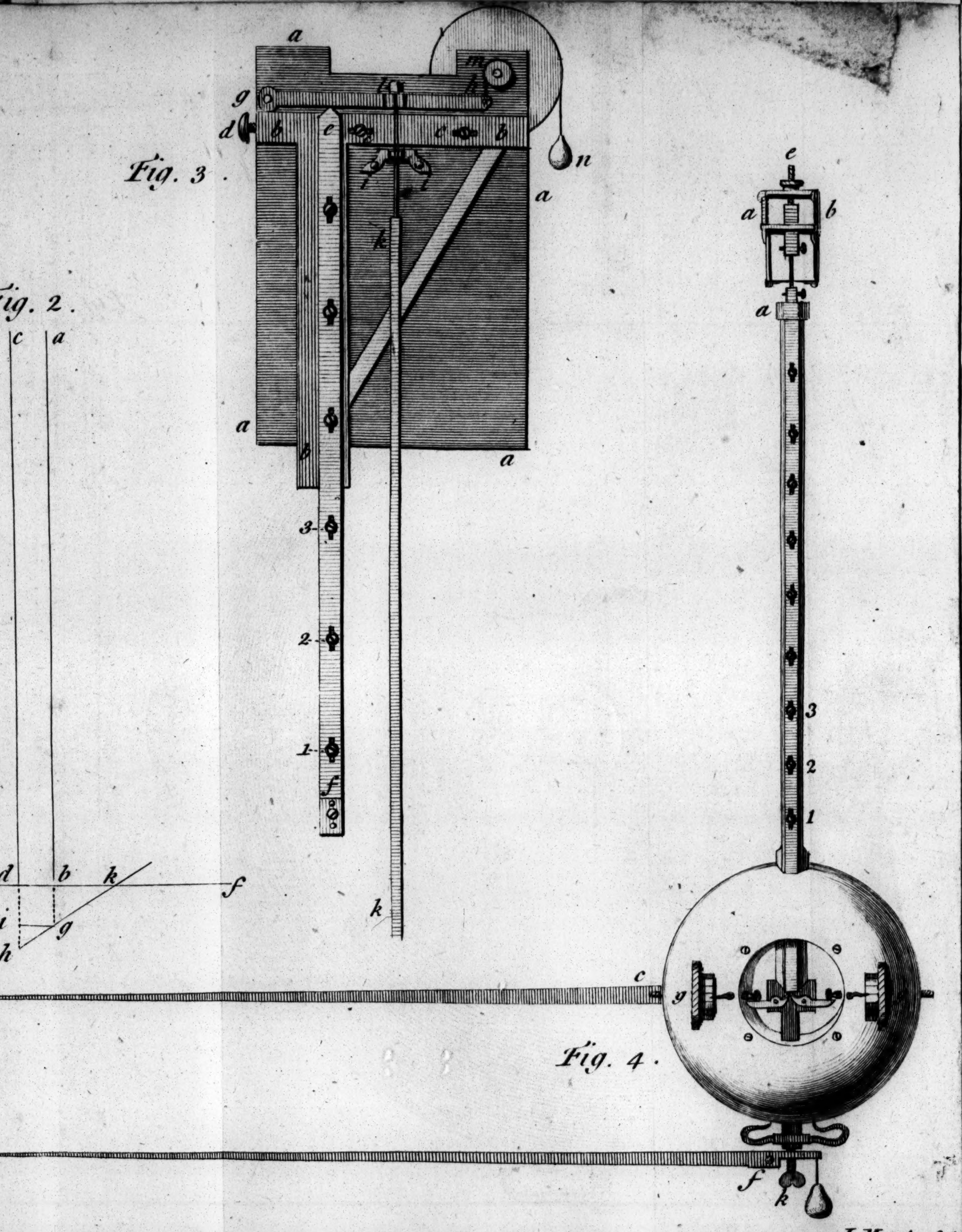


Fig. 2.

Fig. 3.

Fig. 4.

J. Mynde sc.

A Deſcription of Two Methods, by which the Irregularities in the Motion of a Clock, ariſing from the Influence of Heat and Cold upon the Rod of the Pendulum, may be prevented; by John Ellicott. *Read at the Royal Society*, June 4, 1752.

THE firſt of theſe methods conſiſts in a particular conſtruction of the pendulum itſelf, which occurred to me many years ago. In the beginning of the year 1738, I put into the hands of Mr. Machin, then one of the ſecretaries, a deſcription and a drawing of ſuch a pendulum, in order to their being laid before this honourable Society: but Mr. Machin ſoon after acquainting me, that a gentleman, of whoſe ſkill and judgment in mechanical contrivances I had always entertained the higheſt opinion, thought it liable to ſome objections, I was adviſed to defer communicating my invention to this Society, until I ſhould have examined into the weight of thoſe objections, and by a fair and impartial trial ſhould be fully aſſured, that the contrivance would anſwer the end propoſed. And having now at length obtained that ſatisfaction, I beg leave to give a ſhort narrative of ſome of the moſt remarkable obſervations I have made during this inquiry, which I hope will not prove unacceptable to this honourable Society.

About the year 1732, an experiment which I made, in order to ſatisfy ſome gentlemen, that the rod of a pendulum was liable to be conſiderably influenced by moderate degrees of heat and cold, led me to conſider, that as metals differ from each other in their denſity, it was highly probable they might likewiſe differ from each other in their expanſion; and that this difference of the expanſions of two metals might be ſo applied, as in a great meaſure to remove thoſe irregularities in the motion of a clock, which ariſe from the effect of heat and cold upon the length of a pendulum. With this view, not long afterwards I contrived the pendulum now deſcribed by Fig. 1.

In which *a b* repreſents a bar of braſs, made quite faſt at the upper part by pins, and held contiguous, at ſeveral equal diſtances, by the ſcrews 1, 2, 3, *&c.* to the rod of the pendulum, which is a bar of iron; and ſo far as the braſs bar reaches, is filed of the ſame ſize and ſhape, and conſequently does not appear in the figure; but a little below the end of the braſs bar, the iron is left broader, as at *d d*, for the conveniency of fixing the work to it, and is made of a ſufficient length to paſs quite through the ball of the pendulum to *c*. The holes 1, 2, *&c.* in the braſs, through which the ſhanks of the ſcrews paſs into the iron rod of the pendulum, are filed, as in the drawing, of a length ſufficient to ſuffer the braſs to contract and dilate freely by heat and cold, under the heads of the ſcrews. *e e e e* repreſent the ball of the pendulum: *f f*, two ſtrong pieces of ſteel, or levers, whoſe inner centers, or pivots, turn in two holes drilled in the broad part of the pendulum rod, and their outer ones in a ſtrong bridge or cock, ſcrewed upon the ſame part of the rod, but omitted in the draught, becauſe, when put on, it covers this mechaniſm: *g g* are two ſcrews entering at the edge, and reaching into the cavity near the center of the ball.

The ends of theſe ſcrews next the center are turned into the form repreſented in the drawing, which, preſſing with the weight of the ball againſt the longer arms of the levers, cauſe the ſhorter arms to preſs againſt the end of the braſs bar at *b*. Things being in this ſituation, let us ſuppoſe, that the rod of the pendulum, and the braſs annexed to it, grow longer by heat, and that the braſs lengthens more than the iron of the ſame length; then the braſs, by its exceſs of dilatation, will preſs the ſhort ends of the levers downwards at *b*, and at the ſame time neceſſarily lift up the ball, which reſts upon the long ends of the ſame levers at *f f*, to any proportion neceſſary: And provided the ends of the ſcrews do preſs upon the levers at a proper diſtance from the centers, the ſaid ball will be always kept at the ſame diſtance from the point of ſuſpenſion, notwithſtanding any alteration the rod of the pendulum may be liable to from heat or cold. What this diſtance ought to be, may very nearly be determined, if the difference of the expanſion between the braſs and iron bars is known; for the proportion the ſhorter arms of the levers ought to bear to the longer ones, will always be as the exceſs of the expanſion of the braſs is to the whole expanſion of the iron, as may be thus eaſily demonſtrated.

Fig. 2. Let the Line *a b*, drawn perpendicular to the line *e f*, repreſent a bar of iron; the line *c d* a bar of braſs; the prick'd line *b g*, the expanſion of the iron bar by any particular degree of heat; the prick'd line *d h*, the expanſion of the braſs bar by the ſame degree of heat: Let the line *g i* be drawn parallel to the line *e f*, then will *i h* repreſent the difference of

the expanſion of the two metals: Through the points *h g* draw a right line, cutting the line *e f*, as in *k*; this line may be ſuppoſed to repreſent one of the levers turning upon its center at *g*; *h* the point where the braſs bar acts upon the ſhorter end of the lever, and *k* the point where the ſcrew acts upon the longer end of the lever, which being the place where it interſects the line *e f*, it is evident the ball of the pendulum will be as much raiſed by the lever, as it would have been depreſſed by the expanſion of the iron; but the triangle *i h g* is ſimilar to the triangle *h g k*, and therefore, as *i h*, the exceſs of the expanſion of the braſs, is to *h g*, the whole expanſion of the iron, ſo will *h g*, the ſhorter arm of the lever, be to *g k*, the longer arm of the lever: *Q. e. d.*

At *c*, Fig. 1. is placed a ſtrong double ſpring, whoſe ends preſſing againſt the under edge of the ball, hinder it from bending the braſs bar by its forcible action thereon at the point *h*, which, when the ball is of a conſiderable weight, it might otherwiſe be very liable to do. The deſcription here given is exactly agreable to the original contrivance; and the only alteration, I have ſince made in it, conſiſts in placing the ſcrews *g g* within the ball of the pendulum, as repreſented in Fig. 4.

But as the ſucceſs of this contrivance depended intirely upon the ſuppoſition, that metals were expanded differently by the ſame degree of heat, before I attempted to put it in execution, I thought proper to inquire what experiments had already been made upon this ſubject; when Mr. John Eames, a late very worthy member of this ſociety, put into my hands Mr. Graham's account of his quickſilver pendulum (as it is now commonly called) publiſhed in the Philoſophical Tranſactions, N° 392, which account I found to be introduced by the following paragraph.

" Whereas ſeveral, who have been curious in meaſuring of time, have taken notice that the vibrations of a pendulum are ſlower in ſummer than in winter, and have very juſtly ſuppoſed this alteration has proceeded from a change of length in the pendulum itſelf, by the influence of heat and cold upon it, in the different ſeaſons of the year; with a view therefore of correcting, in ſome degree, this defect of the pendulum, I made ſeveral trials about the year 1715, to diſcover whether there was any conſiderable difference of expanſion between braſs, ſteel, iron, copper, ſilver, &c. when expoſed to the ſame degrees of heat, as nearly as I could determine; conceiving it would not be very difficult, by making uſe of two ſorts of metals differing conſiderably in their degree of expanſion and contraction, to remedy, in great meaſure, the irregularities to which common pendulums are ſubject. But although it is eaſily diſcoverable, that all theſe metals ſuffer a ſenſible alteration of their dimenſions by heat and cold, yet I found their differences in quantity, from one another, were ſo ſmall, as gave me no hopes of ſucceeding this way, and made me leave off proſecuting this affair any farther at that time."

The reading this paragraph proved at that time ſufficient to make me lay aſide all thoughts of ſucceeding in a contrivance, founded upon principles, which a gentleman of ſo great abilities, and known accuracy in making experiments, had after trial judged to be inſufficient. And it was not till about the latter end of the year 1734, that I again reſumed them, on the following occaſion. A gentleman, deſirous to make ſome experiments concerning the expanſion of metals, employed me to make him an inſtrument like one invented by Mr. Muſchenbroek for that purpoſe, which he calls a pyrometer. Upon looking over Mr. Muſchenbroek's experiments, I not only found the difference between the expanſion of ſome of the metals much greater than I expected, but, as I thought (if they were to be depended upon) ſufficient to anſwer my former purpoſe. This led me to conſider the ſtructure of the inſtrument, which Mr. Muſchenbroek made uſe of in his trials, and upon examination I thought it liable to ſome objections, which I imagined would make the reſult of experiments made by his inſtrument very uncertain. I therefore endeavoured to contrive one of a different conſtruction, that might be more to be depended upon. Such an inſtrument I ſome time afterwards compleated, and had the pleaſure to find it ſo far met with the approbation of ſeveral very worthy members of this ſociety, that, at their particular deſire, I drew up a deſcription of it; which was read, and the inſtrument itſelf ſhewn to the Society on the 8th of April, 1736;* and though it was not in every reſpect ſo accurate as I could wiſh, I am fully perſuaded that ſuch experiments, as are carefully made with it, may be depended upon, as very near the truth. Having made a great variety of experiments with this inſtrument upon bars of different metals, as nearly of the ſame dimenſions as poſſible, I found, upon a medium, their ſeveral expanſions by the ſame degree of heat to be as follows:

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* This appears from the minutes of the Royal Society; though the deſcription and manner of that inſtrument was by ſome miſtake placed ſix months later, in the *Philoſ. Tranſ.* Vol. xxxix. No. 443. for October, 1736. The other dates mentioned in this Paper, relating to that inſtrument, are likewiſe juſtified by authentic vouchers, which have been produced by Mr. Ellicott.

Gold	Silver	Braſs	Copper	Iron	Steel	Lead
73	103	95	89	60	56	149

And as I found ſo great a difference between the expanſion of braſs and iron, I immediately determined to make a pendulum after the manner above deſcribed, compoſed of thoſe two metals, and likewiſe ordered a clock to be made with the utmoſt care and exactneſs, and, as I then apprehended, with ſome conſiderable improvements, with which I intended to make the experiments. Theſe were both finiſhed in the begining of the year 1738, and having no reaſon to doubt of ſucceſs, I ſhewed the pendulum to the late Mr. Machin, and gave him a drawing and deſcription of it, in order to its being communicated to the Royal Society; but (as I already have obſerved) objections were made to it, of which the only one that appeared to have any weight was; that it had been found by experiment, that two bars of different metals, ſcrewed together ſo as to be in contact with each other, would not expand regularly and ſmoothly, but by jerks. In order to examine into the force of this objection, I directed two bars of equal dimenſions to be made, one of braſs, the other of iron, of about two feet in length, faſtened together after the ſame manner as the two rods of the pendulum,

lum, which I intended to place ſo, that by acting very near the center of an index of a conſiderable length, even the ſmalleſt alteration in the bars would be made ſenſible, and by the motion of the index I ſhould be able to form a judgment, whether the rods moved regularly and freely, or not: But before this was put in execution I contrived, by faſtening the two bars to the back plate of a clock, not only to make them anſwer the end above propoſed, but, at the ſame time, to lengthen or ſhorten a pendulum of a common conſtruction, in ſuch a manner, as ſufficiently to correct the irregularities ariſing from the influence of cold or heat upon it. The manner of applying them is deſcribed by Fig. 3. in which *a a a a* repreſent the back plate of the clock, *b b b* a triangular piece of braſs, ſcrewed by two ſcrews, through the ſlits *c c*, to the plate, yet ſo that it may be drawn backwards or forwards by means of the ſcrew at *d*. *e f* is a braſs bar, about two feet in length, made faſt at the bottom by a ſcrew and two pins at *f* to an iron one of equal dimenſions, to which it is likewiſe ſcrewed by the ſcrews 1, 2, *&c.* after the ſame manner as the rod of the pendulum already deſcribed; the iron bar is faſtened at the upper end to the triangular piece of braſs, nearly under that part of the braſs bar marked *e*. *g h* is a ſtrong braſs or iron lever, moveable upon a center at *g*, and is ſupported by the upper end of the braſs bar. *i i* is the cock, on which, in a common clock, the pendulum is hung. *k k* part of the rod of the pendulum, whoſe ſpring, paſſing through a line ſlit in the cock *i i*, is faſtened to a ſtud rivetted into the lever at *l*. The ſlit in the cock muſt be made ſo cloſe, as to prevent the ſpring from having any lateral motion in it.

From this deſcription it is evident, that, if the braſs bar expands more than the iron one, it will raiſe up the lever, and conſequently the pendulum, which is faſtened to it; and as the length of the pendulum is only from the center of oſcillation to the under part of the ſlit, through which the ſpring paſſes, the pendulum will be thereby ſhortened; and by making the point of the braſs bar to act upon a proper part of the lever (to which it is capable of being adjuſted by means of the ſcrew *d)* the pendulum may be ſhortened to whatever degree ſhall be neceſſary. To prevent the pendulum from bending the bars, which it would be liable to do, if the ball of the pendulum was of any conſiderable weight, the end of the lever, fartheſt from its center of motion, is hooked to the end of a chain, which is wound about and faſtened to a ſmall pulley at *m*. Upon the ſame arbor, to which this pulley is fixed, is faſtened another pulley, of a much larger diameter, to which is hung, by a ſilk line, the weight or counterpoiſe *n*. By means of this counterpoiſe any part of the weight of the pendulum, that ſhall be deſired, may be taken off from preſſing againſt the braſs bar. And if, upon the end of the arbor, to which the pullies are fixed, is placed an index, ſo as to point to a graduated circle, the leaſt motion of the lever will not only be eaſily perceived, but alſo whether that motion is regular and uniform or not. And upon having, ſome time after, made a clock with this contrivance added to it, I had the pleaſure to find the index not only to move very ſenſibly, but very regularly, and never, that I could perceive, by jerks. And I doubt not, but when the point of bearing of the braſs bar, upon the lever, is once well adjuſted, it will be found to lengthen or ſhorten the pendulum to as great a degree of exactneſs, as any other method whatſoever. But as I have not yet thought of any other method of adjuſting it, except from actual trial in different ſeaſons of the year, I muſt prefer the pendulum to this method, which, from the great eaſe as well as exactneſs, with which it is capable of being adjuſted, will, I think, appear to have much the advantage over any other contrivance yet made uſe of for this purpoſe. The method I take for adjuſting the longer arms of the levers of the pendulum to the ſhorter ones, is deſcribed in Fig. 4.

To a ſtrong poſt, fixed to the wall, is faſtened a ſmall ſhelf, ſupported by two brackets *a*, *b*. In the middle of this ſhelf is faſtened a wire, by the ſcrew *e*; having an hook at the end of it, on which the pendulum is to be hung; below this ſhelf, at the diſtance of near forty inches, is placed the index *c*, *d*, turning freely upon a center; the length of the index from its center is fifty inches. At the diſtance of half an inch, upon a part of the index produced beyond the center, is placed a ſteel pin; and in the back of the pendulum, as near the center of oſcillation as may be, is drilled an hole to receive this pin; when the pendulum is hung upon the hook againſt the poſt, and the hook is either ſcrewed higher or lower by the ſcrew *e*, until the pin reſting againſt the upper part of the hole (which is filed into a proper ſhape for that purpoſe) keeps the index nearly in an horizontal poſition. Below the bottom of the pendulum is placed a ſecond index, *f*, *g*, exactly like the former, except that it is kept in an horizontal poſition by the ſcrew *k*, bearing againſt the end of the iron rod. When the experiment is to be made, the pendulum is firſt put into

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a box, and gradually heated by a large fire, to a conſiderable degree, being often turned, that every part may be equally expoſed to the fire; and having continued ſhut up in the box for ſome time after it is removed from the fire, that the two rods may be heated as uniformly to the ſame degree as poſſible, the pendulum is hung upon the hook, and the two indexes made to ſtand nearly in an horizontal poſition: the two graduated plates *h*, *i*, are then ſlid upon a wire, till the diviſion in each marked *o*, are pointed to by the indexes. As the pendulum cools, the lower index will be ſeen gradually to deſcend; but, if the ends of the two ſcrews, in the ball of the pendulum, act upon proper parts of the levers, the upper index will continue in the ſame place. If the ends of the ſcrews are either too far off, or too near the centers of the levers, the index will either riſe or deſcend; and by comparing the number of diviſions it has varied, with thoſe which the lower index has varied, a near eſtimate may be made, how much the ſcrews required to be altered, and in a very few trials they may eaſily be adjuſted to a very great exactneſs.

In order to make an actual trial how far this contrivance of the pendulum will anſwer the end propoſed, it is neceſſary, that the clock, to which the pendulum is fitted, be made with great exactneſs, and entirely to be relied upon, for otherwiſe the experiments will be very uncertain, as I found in the clock I firſt made uſe of. I have already obſerved, that in order to render this clock as perfect as poſſible, I made it, in ſeveral reſpects, different from the common ones, in hopes of removing ſome imperfections I apprehended they were liable to. But as, in this attempt, I fell into an error, which it was a conſiderable time before I diſcovered, my making the trial was thereby greatly retarded. And in order to prevent others from falling into the like miſtake, I ſhall beg leave to give ſome ſhort account of it.

In a common clock, the pendulum is uſually hung by a ſpring to a cock on the back plate of the clock, whilſt the wheel and pallates, by which the pendulum is kept in motion, are placed in the middle of the frame; and the pendulum is moved by a piece of ſteel (called the crutch) rivetted to one end of the arbor, to which the pallates are faſtened. This diſpoſition of the pieces I apprehend liable to ſome conſiderable objections; to remedy which, I contrived to fix the pallates to the upper part of the pendulum itſelf, above the center of motion; and in order to make the pendulum vibrate as freely as poſſible, it was made to turn upon two ſteel points, and was hung in the middle of the frame, exactly under the ſwing wheel, and ſo as to vibrate in the ſame plane with it. By this means I was in hopes that it would have moved with much greater freedom and regularity, than when hung after the common method; and, upon trial, it was found to move with ſo great freedom, that a pendulum of above twenty pounds weight, when hung in its place, without the clock-work, and made to vibrate through an arch of two degrees, was found to make above twelve hundred vibrations before it had loſt half a degree, and was obſerved to have a ſenſible motion above twenty hours afterwards; and the clock, when firſt put together, was kept going for ſeveral days, by a weight of only eleven ounces, hung to the end of a ſingle line. But it was not long before I diſcovered this great freedom made it liable to be conſiderably affected by the leaſt motion.

A remarkable inſtance of this, I communicated to this Society, which was publiſhed in the Philoſophical Tranſactions, No. 453. But the greateſt objection to this method was, the points being ſubject to wear; and I found that the leaſt alteration in them would occaſion the clock to vary much more than (without having made the trial) I could have imagined. To remedy this inconvenience, I made the pendulum to move on edges, like thoſe on which the beam of a pair of ſcales turns (a method I had good reaſon to believe had been made uſe of with ſucceſs) but I found theſe likewiſe liable to wear, though not in ſo ſhort a time as the points; ſo that after much time ſpent in making ſeveral experiments, in order to remedy this inconvenience, I found myſelf obliged to lay this method wholly aſide, and to hang the pendulum by a ſpring as uſual: in making this alteration I obſerved one circumſtance, which I think deſerves to be taken notice of.

Before I made any alteration in the work, I took particular notice to what height the pendulum required to be raiſed, before the pallates would ſcape from the wheel. I next obſerved the number of degrees of each vibration of the pendulum, when moved by the clock-work; and then, the clock-work being removed, the pendulum was made to deſcribe an arch of two degrees; and particular notice was likewiſe taken, in what ſpace of time it had loſt half a degree each vibration; having then made the neceſſary alterations for hanging the pendulum by a ſpring, and particular care being taken that the pallates ſhould ſcape off from the wheel exactly at the ſame angle as before, the pendulum being hung by its ſpring, and made to vibrate through an arch of two degrees, it was obſerved to loſe half a degree in about half the time it did when turning upon edges: but upon being ſet a going by the clock-work, the pendulum was found to deſcribe an arch of near two degrees more than before; for when it turned upon the edges, it deſcribed an arch of only three degrees, whereas now it was hung by the ſpring, it vibrated near five degrees, which was very different from what I expected.

This alteration being made, I ſoon found that the clock went very regular, and after a ſufficient trial was fully ſatisfied, the pendulum would anſwer my expectations. But fearing leſt I might be thought prejudiced in favour of my own invention, I engaged the reverend Mr. Bliſs to make trial of it; and accordingly, in the beginning of the year 1750, I ſent to him, at Oxford, a clock for that purpoſe; and in January laſt I received from him a letter, giving his opinion of it, of which the following (ſo far as relates to the clock) is an exact copy.

" SIR,

" I have now had thorough trial of the clock, and
" am perfectly ſatisfied that your pendulum takes off
" the effects of heat and cold, as well as either the
" gridiron pendulum, as it is commonly called, or
" the quickſilver pendulum; and this upon ſuffici-
" ent trial for near two years. It has this advantage
" of both the before-mentioned ones, that it may, by
" lengthening or ſhortening the levers, be eaſily ad-
" juſted to the exact proportion of the different ex-

" panſion of the iron and braſs, which neither of thoſe " kinds is capable of, without very great trouble and " difficulty. I was indeed prejudiced againſt the method of doing it by levers, as I had heard the late " Mr. Graham ſay, that he had tried levers in different ways, and that he found they did not work regularly and ſmoothly, but by jerks: However, in " your method, I am ſatisfied by the fulleſt experience, that they ſucceed as well as either of the " other ſorts, or perhaps any other kind that may " be invented hereafter."

Before I conclude this paper, I beg leave to acquaint this honourable Society, that in the year 1748, I made the model of a contrivance to be added to a pocket watch, founded upon the ſame principles, and intended to anſwer the like purpoſe, as the pendulum above deſcribed; and at a meeting of a council of this Society, on the fifteenth of February laſt, I produced a watch (which I had made for a gentleman) with this contrivance added to it, and likewiſe a model, by which was ſhewn to the gentlemen then preſent what effect a ſmall degree of heat would have upon it. But as I have not yet had ſufficient trial of this watch, I ſhall defer giving a particular deſcription of this contrivance, till I am fully ſatisfied to what degree of exactneſs it can be made, to anſwer the end propoſed.

I am, Gentlemen, your moſt obedient Servant,

J. Ellicott.

Copy of a Letter preſented to the Royal Society, *and read at a Meeting of the Society, held* November 9, 1752, *and printed with ſome ſmall Alterations in the 47th Vol. of the* Philoſophical Tranſactions, *Page* 517.

To the Royal Society.

GENTLEMEN,

THE ſubject of converſation, of late, having been often turned upon that ingenious contrivance in the pendulum of a clock, to prevent the inequalities in its motion, ariſing from its different lengths, in different ſeaſons of the year, by the effects of heat and cold; and it having been often aſked who was the inventor of it, I have therefore thought proper to draw up the following hiſtorical account of it; and as this account contains nothing but matters of fact, ſupported by the beſt authorities, I hope it will be acceptable to this Society.

I am your moſt obedient humble Servant,

J. SHORT.

SOON after the invention of pendulum clocks (juſtly aſcribed to the celebrated Mr. *Huygens)* it was found, that they were liable to conſiderable inequalities in their motion, which were imagined to ariſe from the pendulum, in its vibrations, deſcribing an arc of a circle, and conſequently, that the larger vibrations muſt be ſlower than the ſhorter ones. In order to remedy this imperfection, the ſame Mr. Huygens wrote a treatiſe, called *Horologium Oſcillatorium* (a piece of geometry, which does honour to the laſt century) in which he demonſtrates, from the properties of the cycloid, that the vibration of a pendulum, moving in a cycloid, would be performed in equal times, even though the vibrations were unequal. Pendulums therefore were made to vibrate in a cycloid; but great inequalities were ſtill obſerved in the motions of clocks.

We do not read of any attempts, after this, to regulate the motion of clocks, till the year 1726, when Mr. George Graham delivered in to the Royal Society a paper which is publiſhed in the *Philoſ. Tranſ.* N° 392, in which he ſays, that it having been apprehended, that the inequalities in the motion of clocks aroſe from a change of length in the pendulum by the influences of heat and cold, he, about the year 1715, made ſeveral trials, in order to diſcover, whether there was any conſiderable difference of expanſion between braſs, ſteel, iron, ſilver, *&c.* when expoſed to the ſame degrees of heat, conceiving that it would not be very difficult, by making uſe of two ſorts of metals, differing conſiderably in their degrees of expanſion and contraction, to remedy, in great meaſure, the irregularities to which common pendulums are ſubject; he ſays alſo, that from the experiments he then made, he found their differences ſo ſmall as gave him no hopes of ſucceeding that way, which made him leave off proſecuting this affair any more at that time; that, ſome time after, having obſerved an extraordinary degree of expanſion, by heat, in quickſilver, he thought of a proper manner of applying a column of it to the pendulum of a clock, in order to prevent the inequalities ariſing from its different lengths by the effect of heat and cold; which ſucceeded accordingly, and is what is now called Mr. *Graham's* quickſilver pendulum. Mr. Graham in the ſame paper takes notice, that though the pendulum of a clock was to remain invariable, yet there would ſtill be ſome irregularities in the motion of the clock, ariſing from the friction of the different parts of the clock-work, and from the different degrees of foulneſs.

In the year 1725, Mr. John Harriſon, of Barrow, in Lincolnſhire, made ſeveral experiments upon wires of different metals, in order to find their different degrees of expanſion and contraction; for he thought that by a proper combination of wires of two different metals, differing conſiderably in their expanſion and contraction, he might be enabled to keep the center of oſcillation of a pendulum always at the ſame diſtance from the point of ſuſpenſion. In conſequence of theſe experiments he made a pendulum, conſiſting of one ſteel wire, at the end of which is the bob or weight, and on each ſide of this wire, four wires alternately, braſs and ſteel, ſo diſpoſed and contrived as to raiſe the pendulum the ſame quantity as it is lengthened by heat, and to let down the pendulum in the ſame proportion as it is raiſed by cold. He made alſo a drawing of a clock, in which the wheels are diſpoſed in a different manner from thoſe then in uſe, which drawing I have ſeen, ſigned by himſelf, in the year 1725; two of theſe clocks with pendulums, as deſcribed above, were finiſhed in the year 1726.

In theſe clocks Mr. Harriſon has made a particular ſort of pallates, ſo as to be almoſt entirely free from friction; for though he had thus happily ſucceeded in his contrivance to prevent the inequalities in the motion of the clock, ariſing from the different lengths of the pendulum by the effects of heat and cold, yet he found there were conſiderable errors ſtill remaining, occaſioned by the friction of the pallates as in the common way: he has alſo ſuſpended the pendulum upon the wall of the houſe, intirely independent of the clock and clock-caſe; for he had obſerved conſiderable alterations in the going of the clock, when the pendulum is ſuſpended as in the common manner: his pendulum vibrates in an arc of about 15°, with a bob of about three pounds, between cycloidal checks, which he himſelf found were neceſſary, though he had never heard of Mr. Huygens's book till after he had made them; he has alſo diſpoſed the force of his pendulum-wheel upon the pendulum, by his ſort of pallates, in ſuch a manner, that the vibrations of the pendulum will not be affected by the different reſiſtance of the air. Upon the whole, this clock is made in ſuch a manner, as to be almoſt intirely free from friction; in conſequence of which he uſes no oil, and therefore there is no neceſſity ever to clean the clock. When he ſettled in London, he ſent for one of theſe clocks from the country, and ſet it up in his houſe in Orange-Street, in the year 1739, where it has ſtood ever ſince, and in all that time has never varied above one minute from the truth; he can depend upon it to a ſecond in a month.

About the year 1729, Mr. Harriſon made his firſt machine for meaſuring time at ſea, in which he has likewiſe applied this combination of wires of braſs and ſteel, to prevent any alterations by heat and cold. In the year 1735, he went on board one of his majeſty's ſhips of war with this machine to Liſbon, and returned the ſame year to London, where this machine was ſeen by every curious and ingenious perſon who was pleaſed to go to his houſe. Since that time he has made two more of theſe machines or clocks for keeping time at ſea, in both which he has likewiſe this proviſion to prevent the effects of heat and cold. A pretty good account of theſe curious machines, and of the many ingenious contrivances which Mr. Harriſon has made uſe of in them, for anſwering their intended purpoſe, and alſo an account of the ſucceſs of his voyage to Liſbon and back again, is contained in an excellent ſpeech of our worthy preſident Martin Folkes, Eſq; upon his delivering to Mr. Harriſon the gold medal of Sir Godfrey Copley, which ſpeech is inſerted in the minutes of the Society in the year 1749.

Mr. John Shelton, who was the principal perſon employed by Mr. Graham, in making of aſtronomical clocks, informs me, that Mr. Graham, in the year 1737, made a pendulum conſiſting of three bars, *viz.* one of ſteel, between two of braſs, and that the ſteel bar acted upon a lever, ſo as to raiſe the pendulum when lengthened by heat, and to let it down when ſhortened by cold; this lever, which is very ſtrong, reſts upon a roller, which roller is made moveable, ſo as to adjuſt the arms of the lever to their true proportion; the whole was made to be as free from friction as poſſible in ſuch a conſtruction. Mr. Graham made obſervations, by the tranſits of the fixed ſtars, of the motion of the clock with this ſort of pendulum, and from the experience of ſeveral years (during which the clock was kept conſtantly going) he found that the clock was liable to ſudden ſtarts and jerks in its motion:

of this he informed Dr. Bradley, Mr. Bliſs, and myſelf, and ſeveral other gentlemen. This clock ſtill remains in Mr. Graham's houſe, in the poſſeſſion of his executors.

I have been informed that one Mr. Frotheringham, a quaker, of Lincolnſhire, cauſed a pendulum to be made conſiſting of two bars, one of braſs and the other of ſteel, faſtened together by ſcrews, with levers to raiſe or let down the bob, and that theſe levers were placed above the bob; this clock I have ſeen, and was told * that it was brought from Lincolnſhire to London without a dial plate; the dial plate of it was engraved at Mr. Siſſon's houſe, in the year 1738, and this clock is in the poſſeſſion of Mr. Gibſon, in Newgate-Street, who has had it ever ſince the year 1739.

In the *Hiſtory of the Royal Academy of Sciences at Paris*, for the year 1741, there is a memoir of Mr. Caſſini, in which he deſcribes ſeveral ſorts of pendulums for clocks, compounded of bars of braſs and ſteel, and applies a lever to raiſe or let down the bob of the pendulum, by the expanſion or contraction of the bar of braſs. He has alſo given us, in the ſame memoir, a problem for finding the proportion which the two arms of the lever ſhould have, to anſwer the intended purpoſe; and alſo a demonſtration of it.

In June 1752, Mr. John Ellicott gave in to the Royal Society, a paper containing the deſcription of a pendulum, conſiſting of two bars, one of braſs, and the other of iron, faſtened together by ſcrews, with two levers in the bob of the pendulum, ſo contrived as to raiſe and let down the bob by the expanſion and contraction of the braſs bar, and alſo to adjuſt the arms of the levers to their true proportion.* He ſays, that he firſt thought of this method of applying bars of braſs and iron, to prevent the irregularities of a clock, ariſing from the different lengths of the pendulum by the effects of heat and cold, in the year 1732, and that he put this his thought into execution in the year 1738.

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* In the printed Tranſactions, inſtead of, "That it was brought from "Lincolnſhire to London without a dial-plate," is inſerted, "I was "told by the maker, Mr. John Berridge, that the pendulum of it was "made in the year 1738 or 1739."

In the year 1743, I bought a clock of Mr. Graham, which he had kept going for two years before; this clock has a pendulum compounded of wires of braſs and ſteel, in the manner of Mr. Harriſon's combination; it has alſo a proviſion in the bob to adjuſt the wires, in caſe they happen to be too long. When I firſt took notice of this contrivance or proviſion in the bob, I aſked Mr. Graham the reaſon of it, who told me, that having obſerved ſome inequalities in the motion of the clock, he imagined that they aroſe from the wires being ſomewhat too long, and therefore added this contrivance to adjuſt the length of the wires; but that when he had done this, he found inequalities ſtill remaining, and therefore juſtly concluded that they aroſe from the difference in the friction of the different parts of the clock-work, occaſioned by the differences in the fluidity of the oil, *&c.*

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* The following Note is added in the Tranſactions: "He has alſo "given us, in the ſame paper, another conſtruction of a pendulum to "prevent the effects of heat and cold, conſiſting of two bars, one of "braſs, and the other of iron, the braſs bar acting upon a lever, at the "end of which is faſtened the pendulum; the whole ſo conſtructed and "contrived, as to raiſe the pendulum when it is lengthened by heat, and "to let it down when ſhortened by cold."

From what has been ſaid above it appears, that the improvement of clocks, by a contrivance to prevent their inequalities, ariſing from the different lengths of the pendulum, in different ſeaſons of the year, by the effects of heat and cold, was firſt thought of and executed by Mr. George Graham, and that the application of wires or bars of two metals, which have different degrees of expanſion or contraction, to prevent the ſame inequalities, was alſo firſt thought of by Mr. Graham, and firſt executed by Mr. John Harriſon.*

Some Remarks upon Mr. Short's *Hiſtorical Account of the Contrivances in the Pendulum of a Clock, to prevent the Inequalities in its Motion, ariſing from its different Length, in different Seaſons of the Year, by the Effects of Heat and Cold. By* E. Sparke.

Read December 7, 1752.

HAVING lately been in company where there was read a copy of a paper, ſaid to have been addreſſed to the Royal Society by Mr. James Short, concerning the improvement of pendulums, and perceiving therein, as I thought, ſome tokens of partiality, or at leaſt imperfection, I obtained the peruſal of that copy.

And finding, upon conſideration, that moſt, if not all the facts mentioned therein, and eſpecially thoſe which are really ſupported, as he ſays, by the beſt authorities, ſeem to be collected from the Tranſactions, minutes and papers of the ſame Society, I could not perſuade myſelf that Mr. Short's principal view, in drawing up this very brief hiſtorical account of inventions, was for the better information of the Society; becauſe every member may be ſuppoſed to be as well acquainted with thoſe facts, as himſelf.

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* In the Tranſactions is added, "Without the leaſt knowledge of what "Mr. Graham had done before him."

Upon farther conſideration of the timing this paper, and of the very conciſe hiſtory given of the inventions of a member of the Society, and that he is not ſo much as named as an inventor at the concluſion; and being credibly informed that it has been given out, that the ſame worthy member had laid claim to an invention which he had borrowed or learned from others; it was judged by many of Mr. Ellicott's friends, as well as myſelf, that Mr. Short's paper had a great tendency to throw a tacit blemiſh, or put a ſlight upon a performance, which he (Mr. Ellicott) had the honour to explain to the Royal Society this preſent year; and to deprive him of ſome part of that juſt reputation, which he has ſo deſervedly acquired; and that, for theſe reaſons, that paper ought not to paſs without notice, preſuming that the author himſelf would rather chuſe to have his paper fairly examined, than lie under the leaſt imputation of haſtily running out of his way, for private ends, to undertake a taſk to which he had no call, and for which, if equal, he ſeems by the performance to have been very much unprepared.

Wherefore I hope I may have leave to offer ſome remarks, without giving offence to the Royal Society, or to any of its members.

I ſcarce need to mention the great advantages accruing to the public from accurate hiſtories of uſeful inventions, nor the benefit of finding ſuch hiſtories collected in the Tranſactions of the Royal Society; but think that whoever undertakes any ſuch hiſtory, with a view to being of any real uſe to the publick, ſhould be thoroughly acquainted with the ſubject, and the riſe and progreſs of ſuch inventions; and ſhould have collected accounts of all, or as much as poſſible, of what has been therein done or attempted, by whom, and when; and alſo ſhould be able to judge critically, and give a very particular, true and impartial account of the merits, miſtakes, excellencies and defects of each author, performer, and their performances.

Mr. Short's paper contains many obſervations upon imperfections ſaid to be in clocks, and many other matters which, as I apprehend, have no manner of relation to his propoſed hiſtorical account: Theſe fill near one half of the paper; the reſt does not take notice of one half of the number of contrivances, relating to his ſubject, *which have been made publick.* The following are omitted, which have fallen under my obſervation; but how many more may have been publiſhed, I do not pretend to know.

1. In the month of March, 1724, the Royal Academy of Sciences at Paris examined and approved of a clock, invented by Mr. Henry Sully,* for finding the longitude at ſea; which clock I have ſeen. In a letter to Mr. George Graham, dated June 29, 1724, containing a particular deſcription of this clock, the author,

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* Suite de la Deſcription Abregée d'une Horloge d'une nouvelle Invention, pour la juſte meſure du Temps ſur Mer. By Henry Sully. Printed at Paris, 1726. page 74. (The firſt letter from Mr. Graham to the author Mr. Sully.)

amongſt other improvements therein mentioned, aſſerts, that he has prevented the variations, how ſmall ſoever they may be, ariſing from the dilatation and contraction of metals by heat and cold. Mr. Graham, in his anſwer, dated July 21, 1724, acquaints Mr. Sully, that he had read over, with great ſatisfaction, the deſcription he had ſent him of his ingenious diſcovery for the exact meaſuring of time at ſea, and declares that it by far ſurpaſſed all that had been before attempted for that purpoſe, and gives it as his opinion, that the different degrees of heat and cold will affect it but little, eſpecially if the contraction and dilatation of the pallates and balance wheel are proportional.

2. Another author * informs me, that Mr. Mairan made an attempt to remedy the irregularities which heat and cold cauſed upon the rod of a pendulum, but mentions not the time; though from ſome circumſtances it appears to be before the year 1733.

3. Monſ. Regnauld,† a clockmaker at Chalons, in 1733 contrived a pendulum, the rod of which was of two metals, which, it is ſaid, rectified itſelf.

4. In the year 1735, the reverend Dr. Charles Maſon,‡ of Trinity college, Cambridge, having thoroughly ſatisfied himſelf of the quantity of

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* Traité de l'Horlogerie Mechanique et Pratique, approuvée par l'Academie Royal des Sciences. By Thiout L'Ainé. Printed at Paris, 1741. Vol.I. p.113.

† The ſame. p.112.

‡ A private letter to Mr. Ellicott.

the error, and the inequality of it in common pendulums, had ſeveral different deſigns, about adjuſting the length of a pendulum; one of which was, the making uſe of the different expanſion of two metals; and in 1738 he made a model of that which he thought moſt likely to ſucceed: but I am not certain how far this *has been made publick.*

5. In 1738, Mr. Julien le Roy,|| at Paris, preſented to the Academy of Sciences a contrivance for the ſame purpoſe.
6. In 1739 Monſ. Deparcieux* preſented to the Academy of Sciences a frame, compoſed of braſs and ſteel, which he had got made, in order to procure to clocks the greateſt exactneſs poſſible, by correcting the lengthening and ſhortening, which heat and cold produce in the rods of the pendulums.
7. A ſecond contrivance of the rod of a pendulum, by Monſ. Deparcieux, which ſeems to be an improvement of Mr. Regnauld's pendulum.
8. Mr. Thiout in 1739 mentions † another method which he had uſed in a clock, made for Mr. Le Monier, which he ſays was obſerved to vary but four ſeconds *per* day, between the greateſt degrees of heat and cold in 1739 and 1740.
9. I have likewiſe ſeen a deſcription of a pendulum invented by Mr. Varinge, ⸸ formerly profeſſor of

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|| Monſ. Thiout's Traité de l'Horlogerie. Vol. II. p. 271. Memoires de l'Academie Royal, 1741. Abbé Nolet's Lectures. Vol. IV p. 365.

* Monſ. Thiout's Traité de l'Horlogerie. Vol. II. p. 265 & 268.

† Ibid. p. 273.

⸸ Memoire ſur l'Horlogerie, par M * * *, 1750.

philoſophy at Luneville, and which he made uſe of in his clocks, but without any date.

10. In the Journal des Sçavans, for June 1752, we are informed that Mr. Rivas,* a native of Valais, has lately had a patent granted to him for fifteen years, for the ſole making of a clock, of a new invention, in which is mentioned his metallic compoſition for preventing the lengthening and ſhortening the pendulum by heat and cold.

The accounts of the contrivances which Mr. Short has mentioned, appear to me to be conſiderably defective. The firſt is Mr. Graham's quickſilver pendulum, publiſhed in 1726, invented in 1715, in his own words, as I ſuppoſe, without any illuſtration: no eulogium upon ſuch a beautiful invention, no deſcription how it is contrived and applied, nor the leaſt notice of its ſeveral excellencies and defects.

The next is Mr. Harriſon's pendulum, ſometimes called the *gridiron pendulum*, invented in 1726, mentioned to the Royal Society in a ſpeech 1749; and of this we have an account equally defective with the former, as I think; which defect is not in the leaſt ſupplied by his account of other curious contrivances in Mr. Harriſon's clocks, not relative to the propoſed hiſtory. Here I muſt take notice of the concluſion of Mr. Short's paper, which, as now expreſſed, ſeems to me to imply that Mr. Harriſon executed Mr. Graham's thought, whereas I believe there is no manner of doubt but that Mr. Harriſon put his own thoughts in execution, and was the original inventor and performer of the gridiron pendulum, without any hint or help from Mr. Graham or any other.

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* Memoire ſur l'Horlogerie, par M * * *, 1750. and in the Journal des Sçavans.

The next made mention of is Mr. Harriſon's machine for meaſuring time at ſea, invented in 1729, mentioned to the Royal Society in 1749; it is indeed barely mentioned by Mr. Short, without ſo much as taking notice that *there was no pendulum uſed* in that moſt curious inſtrument.

The next in order of time, which ſhould have been next in place, is the memoir of Mr. Caſſini, publiſhed in 1741, without the leaſt explanation of what Mr. Caſſini has deſcribed in the ſaid memoir; and Mr. Short has even omitted to take notice of a contrivance of Mr. Julien Le Roy's, though it is to be ſeen in the hiſtorical part of the very ſame volume, and is therein recommended as the moſt exact and eaſy method for procuring to clocks this rectification which was wanting to them: the model of this contrivance was preſented to the Academy of Sciences, in 1738, as before-mentioned.

The next in courſe, being the next in time of publication, ſhould have been Mr. Ellicott's account of his compound pendulum and other contrivances, invented 1732, executed 1738, publiſhed June 1752; but this being the *only thing that ſeems to be envied*, is not only poſtponed in place, but treated ſo ſlightly as if there were no merit, no invention, nothing new, no improvement. The very title of the paper is altered or ſuppreſſed, for Mr. Short ſays only that the paper contains *A Deſcription of a Pendulum, &c.* whereas the title runs thus, *A Deſcription of two Methods, &c.*

Here I would willingly ſupply what is wanting in this article in Mr. Short's paper, but muſt confeſs that

I am not a ſufficient maſter of the ſubject, to deſcribe, minutely, the perfections of Mr. Ellicott's two contrivances, nor to ſet down exactly wherein they are, or are not, preferable to others. However, I beg leave to take notice of ſome particulars which are moſt obvious, and which, I think, ought not to have eſcaped Mr. Short's notice, if he had been much inclined to inform the publick, and to do Mr. Ellicott juſtice.

1. The whole contrivance and conſtruction of Mr. Ellicott's pendulum, when firſt made, was intirely new, and quite different from any other.
2. The ſtarting and jerking, ſo much complained of by ſome, was, from the very firſt, intirely prevented, by the eaſy remedy of a double ſpring below the ball.
3. The levers, ſcrews, *&c.* are not only placed in the firmeſt manner and moſt ſightly ſituations, but alſo moſt advantageouſly for adjuſting.
4. The additional apparatus is a new and neat contrivance, perfect in its kind, eaſily applied, and anſwers two excellent purpoſes, which, I believe, *have not been provided for* by any other artiſt: the primary uſe being to adjuſt the levers with wonderful facility, to the utmoſt exactneſs, beforehand, in a very ſhort time: the other uſe being to demonſtrate, to every eye, that the pendulum will grow longer and ſhorter ſmoothly and regularly, without any ſtart or jerk whatſoever.
5. This property of operating ſmoothly and regularly has likewiſe been experienced for near two years, and atteſted by a very worthy gentleman, and very capable judge, who was, before ſuch experience, prejudiced againſt the uſe of levers.

6. Mr. Ellicott's ſecond method is the moſt ſimple and leaſt expenſive of any, quite different from any other whatſoever, and very near equal in uſe to the firſt.
7. Mr. Ellicott has, in a moſt open and generous manner, communicated, and fully laid open the whole of both contrivances, ſo that any workman may, if he thinks fit, avail himſelf thereby; which I think the other artiſts, *here*, have not done.

Laſtly, according to the order of time of publication, we ſhould have had the ſeveral intelligences which Mr. Short has now firſt publiſhed, and ſeems to have gathered up ſince Mr. Ellicott's paper was preſented in June 1752; but though Mr. Short has given theſe intelligences the precedence of place, in his paper, this ſeems to be my proper place for taking notice of them.

The firſt intelligence is from Mr. John Shelton, that in 1737 Mr. Graham made a pendulum conſiſting of a ſteel bar between two of braſs, and that the ſteel bar acted upon a ſtrong lever, ſo as to raiſe the pendulum when lengthened by heat, and to let it down when ſhortened by cold; but that it was found, by ſeveral years experience, that this clock was liable to ſudden ſtarts and jerks in its motion. Now this ſeems to me to be equivalent to ſaying that Mr. Graham, in the year 1737, attempted to make a pendulum in a way, ſomething reſembling Mr. Ellicott's invention, but did not ſucceed in it. I likewiſe apprehend that any gentleman, who undertook to give an hiſtorical account of theſe inventions, ſhould, upon this occaſion, have been both able and willing to have determined and informed his readers, whether the ſtarting and jerking be faults incident to and inſeparable from the uſe of a lever or levers, or whether theſe faults are occaſioned by any (and what) defective or immechanical conſtruction in the particular pendulum in queſtion: I have heard it ſaid, by an excellent mechanic, that the conſtruction is ſuch as gives reaſon to expect ſtarts and jerks in the motion; but as we are told that the clock is in being, I ſuppoſe it may be examined if there be occaſion.

The next intelligence is concerning a pendulum ſaid to have been invented by one Mr. Frotheringham, in the year 1739, conſiſting of two bars, the one of braſs and the other of ſteel, faſtened together by ſcrews, with levers to raiſe and let down the bob, the levers being placed above the bob: this ſeems to be mentioned on account of its reſemblance with Mr. Ellicott's pendulum. Here I think that Mr. Short ought, in juſtice, to have added, that this clock was ſhewn by the maker, John Berridge, to the Royal Society, February 1, 1738-9, on quite another account, without any pendulum; and upon due inquiry he might have been informed, that a very few days afterwards, Mr. Berridge, with Mr. Gibſon, ſaw a clock at Mr. Ellicott's houſe, going, with one of the pendulums deſcribed in his paper; which was ſeveral months before this pendulum, ſaid to have been invented by Mr. Frotheringham, was made and put to the clock; but it has been ſaid, that Mr. Short *declined* or omitted aſking Mr. Ellicott any queſtions concerning the matter of his intended hiſtory, though he was a very likely man to furniſh ſome materials.

The laſt intelligence is, that Mr. Short, in 1743, bought a clock of Mr. Graham with a pendulum (made by Mr. Graham, I ſuppoſe) compounded of wires of braſs and ſteel, in the manner of Mr. Harriſon's combination, with a proviſion in the bob for adjuſting the wires in caſe they ſhould be too long; but has ſaid nothing of its ſucceſs.

Mr. Short has mentioned but one attempt made by Mr. Graham to put in execution his own thought of uſing metals of different expanſions to cauſe a pendulum to continue always of one and the ſame length, and has let us know that he did not ſucceed in it. Are we therefore to conclude that Mr. Graham never made any other attempts, nor ever ſucceeded to his ſatisfaction? I think not.

After theſe curſory obſervations, made on the particulars contained in Mr. Short's paper, it might be thought proper to inquire what ſhould be the occaſion of the whole; but I beg leave to decline that inquiry, leſt it ſhould be deemed invidious, only obſerving in general, that many perſons as well as myſelf are fully perſuaded, that we owe Mr. Short's performance to the paper which Mr. Ellicott had the honour to lay before the Society in June laſt: For I meddle not with any thing that has paſſed in private converſation, or private correſpondence, how true ſoever, or how much ſoever in my power, farther than to take notice of the inſinuation, as if Mr. Ellicott had claimed ſomething as his own, which he had learned or borrowed from ſome others.

I have carefully read over Mr. Ellicott's paper on purpoſe, but cannot find that he has any where laid claim to being the firſt inventor of a method to remedy the irregularities in the motion of a clock, ariſing from the effects of heat and cold on the pendulum, nor any expreſſion tending towards a queſtion, who was the firſt inventor. There is indeed a citation from the Philoſophical Tranſactions, inſerted by him, as I ſuppoſe, to acknowledge, and inform his readers, that Mr. Graham had once thought of making uſe of two metals, of different expanſions, but had ſuſpended the proſecution, having no hopes of ſucceeding; long before Mr. Ellicott had thought of the ſame thing, in which he has ſucceeded. Farther than this, which appears to do honour to Mr. Graham, Mr. Ellicott has meddled with no man's invention, but only has aſſerted his own, every title of which, ſo far as it depends on his own veracity, I dare ſay all perſons, who know him, will very readily believe.

Mr. Short mentions, that Mr. Harriſon finiſhed two clocks in the year 1726, with gridiron pendulums, ſo perfect in that and other reſpects, that one of them has ſtood in his houſe in Orange-Street ever ſince the year 1739, and ſuppoſed, without any rectifying or ſetting, has never varied above a minute from the truth, and that he can depend upon it to a ſecond in a month. I ſhall make no queſtion, but ſuppoſe this to be true upon his own aſſertion *only*, and then I hope I may venture to draw the following concluſions.

That Mr. Graham firſt invented the quickſilver pendulum, and brought it to the perfection it now has.

That the ſame gentleman firſt thought of uſing two different metals, to cure the inequalities in the motion of a clock, ariſing from the effects of heat or cold on the pendulum; but whether he ever ſucceeded to his ſatisfaction in the execution, remains doubtful.

That Mr. John Harriſon afterwards thought of, or invented the ſame thing, and carried it into execution with great ſucceſs (as above lately mentioned) without any information or hint from any other.

Laſtly, That Mr. Ellicott afterwards invented the ſame thing, and carried it moſt happily into execution, with ſeveral advantages, without any aſſiſtance or information from the gentlemen above mentioned, or any other.

To conclude: I ſhall wave recapitulating or ſumming up the evidence, which I apprehend I have produced, whether of imperfections or partiality, not inclining to aggravate at all, but to leave it to every gentleman to make his own reflections; for I have no intent to quarrel with, or any way to prejudice Mr. Short, to whom I am almoſt, if not altogether, an intire ſtranger, but only to obviate the bad effects of a paper, which from the timing, and other circumſtances already mentioned, many were of opinion, was likely to wound the credit of a worthy perſon, whom I rejoice to think I may call my friend.

Obſervations *on ſome* Remarks *on Mr.* Short's *Paper, read* Nov. 9, 1752, *contained in a Paper of Mr.* Sparke's, *read* Dec. 7, 1752. *By* James Short. *With* Notes *by* E. Sparke.

Mr. Short *in this Paper is to take notice of Matters of Fact only, and to endeavour to explain ſome Things, in which he ſeems to be miſunderſtood or miſrepreſented.*

MR. Sparke ſays, in the firſt place of his paper, that moſt if not all the facts, mentioned in Mr. Short's paper, ſeem to be collected from the tranſactions, minutes and papers of this Society, *&c.*

Mr. Short ſays, that he has made only one extract from the tranſactions, *none* from the minutes, and but one from the papers of this Society. (*a*)

Mr. Sparke ſays, that Mr. Short's paper contains many obſervations upon imperfections ſaid to be in clocks, and many other matters which, as he apprehends, have no manner of relation to the propoſed

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(*a*) Mr. Short does not cite Sparke's words exactly; to avoid repetition, ſee page 23 foregoing. Sparke cannot yet perceive but that the principal facts, for which Mr. Short has produced vouchers, or authorities, are, 1. The account of the quickſilver pendulum, from Philoſophical Tranſactions, No. 392. 2. The Preſident's ſpeech, *from Minutes* of the Royal Society, concerning Mr. Harriſon's contrivance. 3. The paper giving a deſcription of Mr. Ellicott's two methods, then in the poſſeſſion of the Royal Society.

poſed hiſtorical account: theſe fill near one half of the paper, &c.

Mr. Short in general has mentioned ſome particulars of the excellent contrivances in Mr. Harriſon's pendulum clock, becauſe they have never as yet been communicated to the public, that what Mr. Harriſon has done in that way may not be loſt, and becauſe without ſuch contrivances a clock can never be made to go true, even tho' the rod of the pendulum was invariable in its length. (b)

The firſt fact mentioned by Mr. Sparke, is of a clock invented by Mr. Sully, and preſented to the Royal Academy of Paris in the year 1724.

Mr. Short has peruſed the volume of the hiſtory of the Academy of Sciences at Paris for the year 1724, and finds mention made of a clock, invented and preſented to the academy by Mr. Sully, but no deſcription or account of the conſtruction of the clock, only that it was made for meaſuring time at ſea. Where Mr. Sparke has ſeen theſe letters, which he mentions, between Mr. Graham and Mr. Sully, Mr. Short knows not, but ſuppoſes they are in *private hands*.

(b) This does not invalidate what Sparke had aſſerted in the places referred to by Mr. Short; neither does it appear to be any ſufficient excuſe for deviating from his ſubject: for as Mr. Short in theſe very obſervations ſays, that Mr. Harriſon is ſoon to give a full deſcription of his pendulum to the Royal Society, and in his former paper had ſaid, " That a *pretty good* account of thoſe curious machines, and of the many ingenious contrivances which Mr. Harriſon has made uſe of in them, is contained in an excellent ſpeech, inſerted in the minutes of the Society in the year 1749:" Sparke cannot but ſtill think, that ſuch general accounts, which fill ſo much of Mr. Short's paper, and have nothing to do with his hiſtorical account, are at beſt quite uſeleſs.

Mr. Sparke gives no author for his ſecond and third facts.

Therefore Mr. Short has nothing to ſay to them.

His fourth fact, concerning Mr. Charles Maſon, Mr. Short ſuppoſes to be private hiſtory, he never having heard of it.

His fifth fact is contained in the hiſtory of the Royal Academy for the year 1741. The four following facts Mr. Short cannot find in the hiſtory of the Academy, and Mr. Sparke has given no author for them.

His tenth fact, ſaid to be in the Journal des Sçavans for the year 1752, Mr. Short has not looked for, Mr. Sparke not having ſpecified the particular volume, but he believes it to be true. (*c*)

Mr. Sparke

(*c*) Sparke has been cenſured for too much aſperity in his remarks, and particularly for inſinuating, *that* Mr. Short had undertaken a taſk, for which, if equal, he ſeemed by his performance to be very much unprepared; for ſaying, in another place, *that* he (Mr. Short) does not take notice of one half of the number of contrivances, relating to his ſubject, which have been made publick; and for ſaying, in another place, *that* Mr. Short declined aſking Mr. Ellicott any queſtions concerning the matter of his intended hiſtory, tho' he was a very likely man to furniſh ſome materials: Therefore it is now ſubmitted, *whether* what Mr. Short himſelf has ſaid concerning the inſtances given by Sparke, *ten* in number, does not prove the juſtice of Sparke's charge, of *haſte*

Mr. Sparke ſays, that Mr. Graham's quickſilver pendulum was invented in the year 1715, and ſeems to complain that Mr. Short has given no deſcription of it, *&c.*

Mr. Short ſays, that Mr. Graham's quickſilver pendulum was invented in the year 1721, as he himſelf tells us in the Tranſactions; and Mr. Short gave no deſcription of it, becauſe Mr. Graham has done it in that Tranſaction. *(d)*

Mr. Sparke ſays, that Mr. Harriſon's pendulum was invented in the year 1726, and that Mr. Short has given an account of it equally defective with the former, *&c.*

Mr. Short ſays, that Mr. Harriſon invented his pendulum in the year 1725. Mr. Short's deſcriptions, through his whole paper, are only general, but Mr. Harriſon is ſoon to give a *full deſcription* of his pendulum himſelf to this Society. Mr. Sparke might have eaſily ſeen, by the dates in Mr. Short's paper, that Mr. Harriſon invented

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haſte and being *unprepared*; and alſo prove that Mr. Short continued unprepared, when he wrote theſe haſty obſervations, as appears by his ſuppoſing ſome of the inſtances to be only in private hands, tho' Sparke had expreſsly ſaid they had been made publick (except the 4th): And *whether* the vouchers now referred to in Sparke's remarks do not prove the ſame charge, and *that*, if Mr. Short had applied to Mr. Ellicott, he might very probably have been ſhewn the paſſages in the ſeveral authors, and perhaps many more.

(*d*) What Sparke has ſaid of Mr. Graham's quickſilver pendulum was taken from Mr. Short's hiſtorical account (in which the year 1721 is not mentioned) not from Tranſactions, No. 392, which he had not by him.

his pendulum, without any knowledge of what Mr. Graham had done before him.(*e*)

Mr. Sparke ſays, that the next fact mentioned by Mr. Short is, Mr. Harriſon's machine for meaſuring time at ſea; it is indeed barely mentioned by Mr. Short, *&c.*

Mr. Short's reaſon for not giving a deſcription of Mr. Harriſon's ſea clocks was, becauſe the ſame is contained in a ſpeech of Mr. Folkes to the Society, to which Mr. Short in his paper refers. His only reaſon for mentioning them was, becauſe wires of two different metals, which have different degrees of expanſion and contraction, were applied in them, to prevent the effects of heat and cold, and that the firſt of theſe machines was ſeen in the year 1736, by many gentlemen, amongſt whom Mr. Ellicott was one, who called more than once at Mr. Harriſon's houſe, and ſaw it about the end of the above year, or the begin-

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(*e*) Here Sparke owns an error of himſelf, or the tranſcriber, and that he ſhould have ſaid *executed* in 1726, invented in 1725, eſpecially as Mr. Short produces a voucher, *viz.* that he (Mr. Short) had ſeen a drawing *ſigned by himſelf* (Mr. Harriſon) in the year 1725, and if that drawing was really made and ſigned in 1725, it ſhould ſeem that Mr. Harriſon was a competitor for fame, before he had executed any thing in this way himſelf, or had heard of any other perſon who had.

That Mr. Short's deſcriptions are only general, Sparke complains in his Remarks, page 24, in the paragraph beginning with, "I ſcarce need to mention, *&c.*" However, what Mr. Short has ſaid of Mr. Harriſon's invention *in general* may poſſibly become *more credible*, when Mr. Harriſon himſelf ſhall have given a more particular deſcription, as here promiſed.

ing of the next, as Mr. Harriſon informs Mr. Short, but never told Mr. Harriſon of any contrivance which he had thought of for a pendulum, and did not make his till after he had ſeen Mr. Harriſon's contrivance. (*f*)

Mr. Sparke ſays, that Mr. Julien le Roy's contrivance is recommended in the hiſtorical part of the volume of the Royal Academy, for the year 1741, as the moſt exact and eaſy method for procuring to clocks this rectification which was wanting to them, *&c.*

Mr. Short ſays, that Mr. Julien le Roy's contrivance, in the hiſtory of the academy for the year 1741, is mentioned there as not ſo convenient as M. Caſſini's conſtruction, becauſe four feet and a half muſt be added to the common height of a clock-caſe, in order to contain this contrivance of M. le Roy's. (*g*)

(*f*) The ſpeech referred to is not, ſo far as Sparke can learn, in print. Here Mr. Short would have us believe, as Sparke ſuppoſes, that Mr. Ellicott learned how to make his pendulum, by ſeeing Mr. Harriſon's ſea clock. Not to inſiſt upon the difficulty of there being *no pendulum* in that ſea clock, Mr. Ellicott himſelf has given a ſufficient anſwer to Mr. Short's and Mr. Harriſon's miſtakes herein, concerning him.

(*g*) It is here in effect allowed, that Sparke's charge concerning the omiſſion of Mr. Julien le Roy's contrivance is juſt. It was brought as an evidence of Mr. Short's partiality, being a paſſage immediately relative to his ſubject, and to which he cannot be ſuppoſed a ſtranger, becauſe he not only quotes Mr. Caſſini's contrivance from the ſame volume (not indeed for the ſake of giving a compleat hiſtory, but probably to depreciate Mr. Ellicott's inventions, becauſe Mr. Caſſini made uſe of a lever, to raiſe and let down the bob of a pendulum) but alſo becauſe, without looking farther into that volume, Mr. Caſſini himſelf, in the very ſame memoir, has given an account of Mr. le Roy's contrivance.

Mr. Sparke ſays, that the very title of Mr. Ellicott's paper is altered or ſuppreſſed, for Mr. Short ſays only that the paper contains a deſcription of a pendulum, *&c.* whereas the title runs thus, *A Deſcription of Two Methods, &c.*

Mr. Short ſays, that as thoſe two methods are the ſame in principle, only differing in conſtruction, and as the one is even owned to be inferior to the other, Mr. Short only mentioned that which had the preference: However, Mr. Short has already added this other method to his paper.

Mr. Sparke ſays, that the whole contrivance and conſtruction of Mr. Ellicott's pendulum, when firſt made, was entirely new, and quite different from any other.

Mr. Short ſays, that Mr. Graham's invention was publiſhed in the year 1726, and that Mr. Harriſon's invention was publicly ſeen in London, in his ſea clock, in the year 1736; and that Mr. Ellicott did not make his pendulum till the year 1738. (*b*)

(*b*) Theſe paragraphs do not in the leaſt invalidate what Sparke has aſſerted; for Mr. Graham's pendulum being made upon a principle quite different, and Mr. Harriſon having ***no pendulum*** in his ſea clock, neither the pendulum of the one, nor the no-pendulum of the other, can any way interfere with Mr. Ellicott's contrivances.

Mr. Sparke ſays, that the property of operating ſmoothly and regularly in Mr. Ellicott's clock, has likewiſe been experienced for near two years, *&c.*

Mr. Short ſays, that in his opinion this clock has not as yet been properly examined, whether it moves ſmoothly and regularly, or by jerks, which Mr. Short can make appear when called upon. *(i)*

Mr. Sparke has ſaid ſomething in his Remarks concerning Mr. Graham: But Mr Short is of opinion, that the character of Mr. Graham is ſufficient to defend his memory againſt any thing which Mr. Sparke has ſaid of him. *(k)*

Mr. Sparke ſays, that Mr. Short upon due inquiry might have been informed, that Mr. Berridge, with Mr. Gibſon, in the month of February, 1738-9, ſaw a clock at Mr. Ellicott's houſe, going with one of the pendulums deſcribed in his paper, *&c.*

Mr. Short ſays, that the information which he received concerning Mr. Frotheringham's pendulum was from Mrs. Gibſon, in whoſe poſſeſſion

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(*i*) Mr. Ellicott having called upon Mr. Short for his reaſons for his being of this opinion, and thoſe being printed with an anſwer together with theſe papers, Sparke takes no further notice of theſe paragraphs.

(*k*) Mr. Short cannot ſhew, that Sparke has ſaid any thing tending to reflect on the memory or character of Mr. Graham; he hopes the direct contrary appears in his *Remarks* very viſibly.

it is, and from Mr. John Shelton, who had ſpoke with the maker of it, Mr. John Berridge; but Mr. Short having lately ſeen Mr. Berridge himſelf, and he appearing doubtful, whether that pendulum was made in the year 1738, or 1739, Mr. Short has already altered the date of this pendulum, by ſaying in the year 1738 or 1739.(*l*)

Mr. Sparke ſays, that the laſt intelligence is, that Mr. Short, in 1743, bought a clock of Mr. Graham, with a pendulum compounded of wires of braſs and ſteel, with a proviſion in the bob for adjuſting the wires, in caſe they ſhould be too long; but has ſaid nothing of its ſucceſs.

Mr. Short ſays, that he has given in his paper an account of the ſucceſs of this experiment, made by Mr. Graham, in adjuſting his pendulum.

Mr. Sparke ſays, that Mr. Ellicott afterwards invented the ſame thing, *viz.* the method of applying two different metals of different expanſions to the pendulum of a clock, and carried the ſame moſt happily into execution, with ſeveral advantages, without any aſſiſtance or information from Mr. Graham, or Mr. Harriſon, or any other.

(*l*) The alteration made by Mr. Short, after having ſeen the maker, Mr. John Berridge, *viz.* The changing 1739 into 1738 or 1739, is not a ſufficient correction to anſwer what was objected, *viz.* That the clock was ſhewn to the Society, February 1, 1738-9, without the pendulum, and that the maker, Mr. Berridge, ſaw one of Mr. Ellicott's pendulums, before this was brought to town.

Mr. Short ſays, that whether Mr. Ellicott had any aſſiſtance or information from others, and whether Mr. Ellicott ever ſaw or received an explanation of Mr. Harriſon's combination of wires of braſs and ſteel, and alſo of his levers, before he made his pendulum in the year 1738, will beſt appear by a letter from Mr. Harriſon to Mr. Short, herewith delivered. (*m*)

(*m*) Tho' Mr. Ellicott has already anſwered this paragraph in his *Reply*, I cannot forbear taking notice, that in the deſcription of his pendulum he aſſerts, that he invented it in the year 1732, and acquaints us what led him into the thought of it, what induced him to lay aſide proſecuting this contrivance at that time, and afterwards to reſume it again in 1734, long before it is pretended he had ſeen Mr. Harriſon; and as no attempt is made to contradict theſe poſitions, Sparke thinks we are bound to believe that Mr. Ellicott has given a true account of his invention, at leaſt till ſome probable evidence ſhall be produced to the contrary.

Mr. Harriſon's *Letter to Mr.* Short.

SIR,

AFTER my return from Liſbon, *viz.* in the year 1736, ſeveral gentlemen, clockmakers, *&c.* came to ſee my machine, and amongſt them Mr. Ellicott, and that more than once, and to whom I then not only explained the manner in which the heat and cold is accounted for in the machine, *viz.* from the combination of wires of braſs and ſteel, commonly called gridirons, which are in it, together with their levers, *&c.* but alſo from theſe other gridirons I have here ſent you, the which I oft took in my hand, the better to explain the matter to him; I alſo at the ſame time gave him an account of my pendulum.

One of theſe gridirons becomes ſhorter by heat, and the other two longer, and the effect of them (in the machine) is multiplied by levers, in as nice a manner as poſſible, as with but a very ſmall, and that a very equable drag of weight upon them, having nothing to do with the different ſtrength of any ſpring, as occaſioned by the effects of heat and cold, (the which is very conſiderable) yet ſtill it is a bad method.

Am, Sir, your moſt humble Servant,

Orange-Street,
Dec. 12, 1752.

JOHN HARRISON.

Mr. Ellicott's *Reply to Mr.* Short's *Inſinuations, &c. contained in his Paper, read* Nov. 9, 1752.

AS I would avoid all controverſy ſo far as is within my power, which I am ſenſible is very diſagreeable to this honourable Society, I ſhall only endeavour to vindicate myſelf from ſome inſinuations in Mr. Short's paper, read to the Society on Thurſday laſt, as if I had borrowed the firſt deſign of the contrivance of my pendulum, *&c.* from ſome other perſon, and in particular from Mr. Harriſon.

I think there is no one who ſhall conſider the conſtruction of both the methods deſcribed in my paper, but will own that they are intirely different from any of Mr. Harriſon's contrivances, and have nothing in common with them, except that the effect produced ariſes from the difference in the expanſion of two metals.

That I had diſcovered this property long before I knew Mr. Harriſon, or any of his contrivances, will appear from my paper: But as my veracity is called in queſtion on this occaſion, I would beg leave to lay before the Society, in as conciſe a manner as poſſible, ſuch proofs as I believe will appear inconteſtable, not only with regard to the experiments I have made on the different expanſion of metals, but likewiſe to the particular contrivance of my pendulum.

I took notice in my former paper, that I had laid aſide my firſt thoughts of making a pendulum of two metals in the year 1732, upon reading the account of

Mr. Graham's experiment, publiſhed in the Philoſophical Tranſactions, No. 392, and did not reſume them till after I had made a ſufficient number of experiments with the Pyrometer I had contrived.

When I firſt invented that inſtrument, and with what view, will appear from the following extract of a letter, I have lately received from the gentleman who beſpoke the firſt of theſe inſtruments that was made.

He therein informs me, that in the year 1734 he deſired a friend at Leyden to procure him a Pyrometer, like that of Mr. Profeſſor Muſchenbroek; but that in a letter, dated October 12, 1734, he is informed, that Dr. Deſaguliers had made ſome improvement in that inſtrument, and that the Dutchmen received them from London. — My friend proceeds: "Upon this diſappointment I had immediate recourſe to you, with the figure and deſcription of Mr. Muſchenbroek's machine; you was as quick at diſcovering its imperfections, as in the thought of conſtructing one upon a more ſimple principle, and yet to be of greater exactneſs in its operation. You kindly made the overture to finiſh an inſtrument upon ſuch a plan, without any expence for your trouble or any ſuch improvement. I gladly embraced your generous offer, and conſented to your making all ſuch experiments, as might be conducive to the valuable end you propoſed to yourſelf, which I then (as I have ever ſince) preſumed was in order to find out ſome method of rendering a pendulum leſs ſuſceptive of influence by heat and cold, and to render the motion of a clock moſt exact. In February, 1735-6, you ſent me the Pyrometer, after having compleated all your neceſſary experiments by it."

In a letter from Mr. Gerſten, profeſſor of mathematicks in the univerſity of Gieſen, dated Feb. 12, 1736, N.S. he writes: "Your new-invented machine I think very curious and uſeful for natural philoſophy." And in another letter from the ſame gentleman, dated May 30, 1736, N.S. "I am glad of the ſucceſs of your pyrometer, and you will oblige me very much, if you do not forget to communicate the account of it according to your promiſe.

In a third letter, dated Auguſt 26, 1736, N. S. from the ſame gentleman, acknowledging the receipt of ſome things I had ſent him from London the fifth of June. "I am much obliged to you for the draught and deſcription of your new invented pyrometer. I wanted only the true and accurate meaſure of the ſtandard bar, and of thoſe that have been tried, and of the diameter of the cylinder that keeps the index. I ſhall ſend you another time a full account of my thoughts, how this machine might be improved, and likewiſe of the pendulum." This account I received in a letter, dated February 19, 1737, in which the conſtruction of the pendulum is ſufficiently deſcribed, to prove it to be the ſame with that deſcribed in my paper.

The original letters ſhall be produced if deſired, and I hope theſe extracts ſufficiently prove what I have undertaken.

Mr. Short ſays, that whether Mr. Ellicott had any aſſiſtance or information from others, and whether Mr. Ellicott ſaw or received an explanation of Mr. Harriſon's combination of wires of braſs and ſteel, and alſo of his levers, will beſt appear by a letter from Mr. Harriſon: — So much ſtreſs is laid upon this letter, that I muſt beg leave to make ſome remarks upon it.

Mr. Harriſon aſſerts in this letter, that in the year 1736 I was at his houſe, and that more than once, and that then he not only explained to me the manner in which the heat and cold is accounted for in the machine, *viz.* from the combination of wires of braſs and ſteel, commonly called gridirons, which are in it, together with their levers, but alſo from ſome other gridirons, which he took in his hand, the better to explain the matter, and that at the ſame time he gave me an account of his pendulum.

I would juſt obſerve, that were theſe aſſertions true, they would not prove the thing for which they are produced, unleſs Mr. Short could ſhew I had copied any thing from Mr. Harriſon's contrivances, which I hope I have already proved I had not: But in every one of theſe aſſertions I am fully perſuaded that Mr. Harriſon is miſtaken.

That I was at that gentleman's houſe more than once I acknowledge, but from ſome particular circumſtances, I have ſince been able to recollect, I have good reaſon to think it could not be in the year 1736, but near the end of the year 1737, or the beginning of the year 1738, and then Mr. Harriſon refuſed to explain to me in what manner his combination of braſs and ſteel wires acted, nor did I ever ſee any of his gridirons as he calls them, except thoſe in his clock, and I am to this time quite a ſtranger in what manner they are applied: Neither do I in the leaſt remember, that Mr. Harriſon ever mentioned his pendulum to me, but think I have ſufficient proof to convince any impartial perſon, that the firſt account I had of it was in a letter from a gentleman, dated November 30, 1740.

The laſt paragraph in Mr. Harriſon's letter is a very remarkable one, but as that concerns himſelf more than me, I ſhall forbear making any obſervations on it.

Mr. Short gives it as his opinion, that this clock has not as yet been properly tried, whether it moves ſmoothly and regularly, or by jerks; which ſeems to me a very ſevere cenſure on the reverend Mr. Profeſſor Bliſs, who made trial of it for near two years, and has lately informed me, that he is very well ſatisfied with the trial he had made, and is fully able to anſwer all the objections, which he has heard againſt it, to the ſatisfaction of all unprejudiced perſons. JOHN ELLICOTT.

Mr. Ellicott*'s Letter to Mr.* Short.

SIR,

YOUR Hiſtorical Account, *&c.* being printed in the laſt volume of Philoſophical Tranſactions, and no notice taken in the ſame volume of Mr. Sparke's Remarks on your Paper, nor of your Obſervations on thoſe Remarks, nor of my Reply, I intend to print the whole with explanatory notes and vouchers: And taking notice that in your Obſervations there is the following paragraph, *viz.* "Mr. Short ſays, that in his opinion "this clock has not as yet been properly examined, "whether it moves ſmoothly and regularly, or by "jerks, and which Mr. Short can make appear, when "called upon," I think it but fair to call upon you accordingly, to furniſh me with what you think proper in ſupport of what you have aſſerted in that paragraph, that the whole may be anſwered at once.

May 9, 1753.

I am your humble Servant,

JOHN ELLICOTT.

Mr. Short's *Anſwer.*

Surrey-Street, May 17, 1753.

SIR,

I SEND you the following anſwer to your letter of the 9th of May, in which you call upon me to give the reaſons of my opinion, that your clock has not as yet been ſufficiently examined, whether it moves ſmoothly or by jerks. Soon after your paper, concerning the conſtruction of two ſorts of pendulums with levers, was read in the Royal Society, I wrote to the reverend Mr. Profeſſor Bliſs (who had examined a clock with one of them applied to it) deſiring he would ſend me twelve obſervations, *viz.* one for each month in a year, in order to compare the going of your clock with one which I had bought of Mr. Graham in the year 1743. Mr. Profeſſor was pleaſed, in anſwer to mine, to ſend me a letter, in which is the following paragraph.

" The clock which I have examined goes but ſeven " days, two hours and a half, which by reaſon of ſick- " neſs, and other avocations from home, has occa- " ſioned it to go often down and ſtand; ſo that it " would be of no uſe to ſend you the obſervations you " deſire, except it had continued going without inter- " ruption for a year."

Now, Sir, it is my opinion, that every machine, which is intended for the nice menſuration of time, ought to be thoroughly examined, and that this examination ſhould be continued without interruption for the ſpace of a whole year at leaſt; becauſe by this means we may be ſatisfied in what manner the machine is affected, in the different ſeaſons of the year, by the effects of heat and cold, moiſture and dryneſs: And this, in my humble opinion, ought to have been particularly attended to in the examination of your clock, becauſe there are obvious objections, at leaſt to me, againſt the conſtruction of its pendulum, ariſing from the friction of the levers, *&c.*

This opinion of mine is alſo founded upon the judgement of the late Mr. George Graham, who was univerſally allowed to excell, not only in his own buſineſs as a watch and clock maker, but likewiſe in every other branch of mechanicks, who ſays, in the Philoſophical Tranſactions, No. 392, That when he was examining his clock with the quickſilver pendulum, " the clock " was kept conſtantly going, without having either " the hands or pendulum altered, from the ninth of " June 1722, to the fourteenth of October 1725, being " three years and four months."

I would not have you, Sir, to ſuppoſe, that I affirm that your clock moves by jerks: All that I ſay is, that it does not appear, to me at leaſt, that your clock has as yet been ſufficiently examined, whether it moves by jerks or not.

I am, Sir, your very humble Servant,

JAMES SHORT.

Mr. Bliſs's *Letter to Mr.* Short.

Oxford, July 12, 1752.

DEAR SIR,

YOUR letter of the ſixteenth of June came ſafe to hand, which ſhould have been anſwered before this time, if the ſudden death of my brother in Glouceſterſhire had not called me hence into that country, and put it quite out of my head. That I have examined a clock of Mr. Ellicott's conſtruction with levers above two years paſt is true, and that I approve of it, is likewiſe as true; and that the obſervations by which I examined it were by tranſits of the ſun, made with an inſtrument of Dr. Bradley, he has rightly informed you. The clock which I have examined goes but ſeven days, two hours and a half, which by reaſon of ſickneſs, and other avocations from home, has occaſioned it to go often down, and to ſtand; ſo that it would be of no uſe to ſend you the obſervations you deſire, except it had continued going without interruption for a year. I find upon examining my book, that the greateſt difference in the going of the clock, between the coldeſt weather of the two laſt winters, and the hotteſt weather of the two laſt ſummers, is no more than one ſecond *per* day; and this was occaſioned by the levers being made too ſhort, of which I advertiſed Mr. Ellicott above a year ſince: Whereas a clock with a ſimple pendulum and braſs rod, made by Mr. Graham, and which belongs to Dr. Bradley, in the coldeſt weather loſt above fifteen ſeconds *per* day, and in the warmeſt gained above thirteen ſeconds *per* day, and went very near the equal time in temperate weather. The pendulum I have tried was the firſt that Mr. Ellicott ever made of this conſtruction, but I expect another clock from him this ſummer, which will be made to go longer, and I will if poſſible keep it conſtantly going a whole year. As ſoon as I have regulated it, I will let you know by a line when I begin to obſerve, and ſhall be glad to exchange obſervations with you at the end of the year; for every uſeful invention that promiſes to ſucceed ought to have a fair and candid trial, without any reſpect to perſons, parties, or friendſhips.

I am, &c.

NATHANIEL BLISS.

Mr. Bliſs's *Letter to Mr.* Ellicott.

Oxford, May 23, 1753.

DEAR SIR,

I HAD the favour of your's of the nineteenth inſtant, with a copy of Mr. Short's letter to you of the ſixteenth, containing his objections to the examination of the clock, whether it moves ſmoothly or by jerks, for which I return my thanks. When I firſt undertook the examination of your clock, I had no other view but to promote uſeful ſcience, by giving a contrivance, that ſeemed to promiſe ſucceſs, a fair and candid trial. When I wrote to you of the ſucceſs of that trial, I was not influenced by any partial motives, or perſonal friendſhip, but paid a ſtrict regard to truth, as far at leaſt as it appeared to me.

I intirely agree with Mr. Short, that "Every machine that is intended to meaſure time nicely ought "to be thoroughly examined, and this examination "ſhould be continued for the ſpace of a whole year "at leaſt; becauſe by this means we may be ſatisfied "in what manner the machine is affected, in the different ſeaſons of the year, by the effects of heat and "cold, moiſture and dryneſs." Now all this has in my opinion been complied with, notwithſtanding the clock has not gone *a whole year without interruption*, but has ſometimes gone down, having been neglected to be wound up; becauſe the clock was examined for almoſt three years before I returned it to you, by the paſſage of the ſun over the meridian, and the regularity of its motion was compared with the motion of the ſun properly equated between one obſervation and another. And if the clock, when let down, be ſet going five or ſix hours before the firſt obſervation is made, ſo that the pendulum may reduce itſelf to its proper ſwing, I can ſee no reaſon why its having ſtood a few hours can at all affect that compariſon. This compariſon has been made as often as the weather would permit, or I was not interrupted by buſineſs or abſence from home. And I can at preſent think of no other way of trying, whether the pendulum moves ſmoothly or by jerks, but by comparing it with obſervations as frequently as poſſible.

I have examined my papers wherein I kept an account of the compariſon of your clock with the tranſits of the ſun, and find that it went without interruption from February the twenty-fourth, 1750-1, to October the thirteenth, 1751, when it was forgot to be wound up. It was ſet going the ſame day, and went till

November the third following, when it was forgot to be wound up ſoon enough the ſecond time. It was ſet going the ſame day, and continued going till January the twefth, 1751-2, when it was again let down for the ſame reaſon. It was ſet going again the ſame day, and went without interruption till July the 5th, 1752. I find alſo by the ſame papers, that I never made any obſervation of the ſun the ſame day that the clock was ſet going; that I ſeldom made leſs than five or ſix obſervations each month, and in ſome months many more; and that I never could diſcover by my obſervations but that the clock went equally regular, and kept exactly the ſame time, after it had been wound up and ſet going again, as it did before it had gone down and ſtopped. If this method of examination is not ſufficient to convince any unprejudiced perſon, it muſt be left to future experiments. And as there are now ſeveral clocks made with pendulums of the ſame conſtruction, which already are, or will ſoon be in the hands of ſkilful perſons both at home and abroad, I make no doubt but that their obſervations will confirm my opinion. It may not be improper to remark, that after my former approbation of the clock, it might not have anſwered Mr. Short's purpoſe, if I had ſent him the obſervations he deſired; becauſe it would have been no difficult matter to have forged ſuch as would have made the clock appear to go to any degree of exactneſs: And therefore the fidelity of the obſerver muſt be entirely relied upon. I ſhall only add, that I have not knowingly or deſignedly impoſed upon you, in the account I ſent you of the going of your clock, and am,

SIR, Your moſt obedient humble Servant,

NATHANAEL BLISS.

APPENDIX.

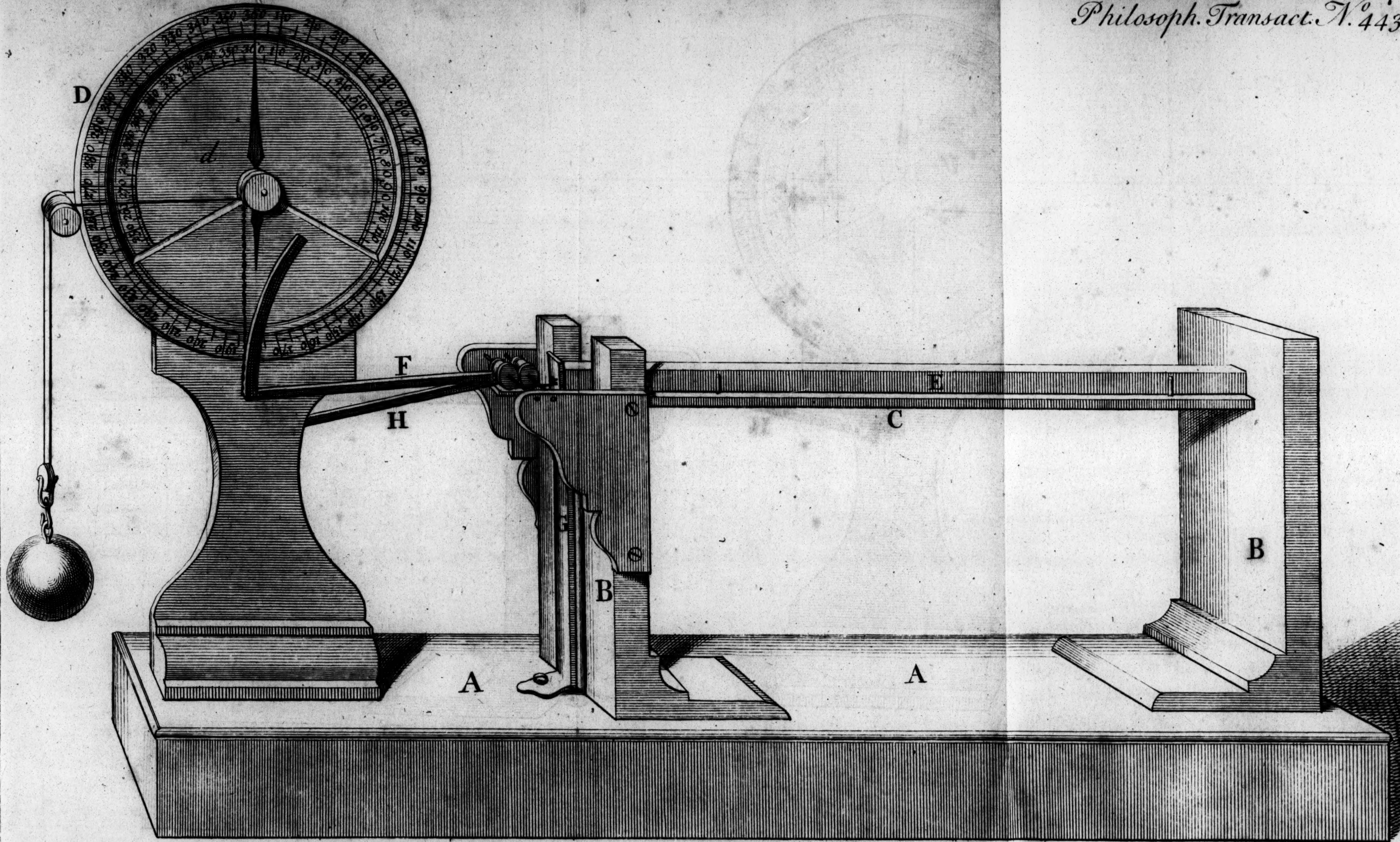
The Deſcription *and Manner of uſing an* Inſtrument *for meaſuring the Degrees of the* Expanſion *of* Metals *by Heat. By Mr.* John Ellicott.

See the Fig. *prefix'd.*

AA is a flat piece of braſs, which, for farther ſtrength, is ſcrewed down to a thick piece of mahogany: upon this plate are ſcrewed three pieces of braſs, two of which, marked BB, ſerve as ſupports for the flat iron bar C; and which, on account of its uſe, I ſhall call the *ſtandard bar*. The upper part of the third piece of braſs is a circle about three inches diameter, divided into 360 equal parts or degrees: within the circle is a moveable plate, divided likewiſe into 360 parts, and a ſmall ſteel index. The braſs circle in the *fig.* is marked D, and the moveable plate *d*. Upon the ſtandard bar the bar of metal is laid, on which the experiment is to be made, as E.

F is a leaver 2½ inches in length, faſtened to an axis, which turns in two pieces of braſs ſcrewed to one of the ſupports marked B: to the end of this leaver is faſtened a chain, or ſilk line, which after being wound round a ſmall cylinder, to which the index in the braſs circle D is faſtened, paſſes over a pulley, and has a weight hung to the end of it: upon

Philosoph. Transact. N.o 443.



J. Mynde sculp.

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upon the axis, to which the leaver is fixed, is a pulley, $\frac{1}{4}$ of an inch diameter, to which a piece of watch-chain is faſtened; the other end of this chain is hooked to a ſtrong ſpring, marked G, which ſpring bears againſt one end of the metal E.

H is a leaver exactly of the ſame form and dimenſions with the former; but the chain faſtened to the pulley on its axis is hooked to the ſtandard bar.* The line faſtened to the end of this leaver, after being wound round a cylinder, to which the moveable plate is fixed, paſſes over a ſmall pulley, and has a weight hung to the end of it; or rather the ſame line paſſing under a pulley, to which the weight is hung, has its other end faſtened to the leaver F: thus one weight ſerves for both leavers, as in the *figure*.

From this deſcription it is plain, that whenever the bar E is lengthened, it gives liberty to the weight to draw the leaver F upwards by its action on the ſpring G; and the index will, at the ſame time, by means of the ſilk line, be carried forward in the circle; and as the bar ſhortens, it will return back again; the ſame motion will be communicated to the ſtandard bar.

The lengthening the bar the $\frac{1}{20}$ of an inch, will carry the index once round the braſs circle, which is divided into 360 degrees; therefore, if the metal lengthens the 7200th part of an inch, the index will move one degree.

To make an experiment with this *inſtrument*, lay a bar of any kind of metal, as E, on the ſtandard bar;

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* *N. B.* The chain to the former pulley being faſtened to a ſpring, and not directly to the metal E, is only for the more eaſy ſhifting the metals.

then heat the ſtandard bar to any degree of heat with a lamp, and mark the degree of its expanſion, as marked by the moveable plate: obſerve alſo the degree of expanſion of the metal E, by the heat communicated to it from the ſtandard bar, as marked on the braſs circle by the index: let the *inſtrument* ſtand, till the whole is thoroughly cold; then removing the bar E, lay a bar of any other metal in its place, and heat the ſtandard bar to the ſame degree of heat as before, which is ſeen by the moveable plates marking the ſame degree of expanſion. Then the index will ſhew the degree of expanſion of the ſecond metal, as it did of the firſt; and, by this means, the degrees of expanſion of different metals, by the ſame degree of heat, may be exactly eſtimated.

FINIS.